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its energy, and that it did not matter how it did it, whether by viscosity or hysteresis or heat conduction or reflection or vortex motion. (I omit divergency because the only functions I can find connected with divergency which would prevent discontinuity either vanish at a short distance from the source, or only exist at the lateral edges of the wave, and hence do not affect spherical waves.)

Now in a fluid like the ether, viscosity, hysteresis and heat conduction losses can not occur. Nor, if my work is correct, can reflection occur without vortex motion, and then not necessarily.

But the vortex motion is a necessity, in a fluid like the ether, whenever a spherical wave reaches a certain distance from its source. And gravitational waves must therefore give rise to vortices in the ether.

And the satisfactory point about these vortices is that they are exactly similar, irrespective of the intensity of the gravitational wave, and dependent only upon the elasticity and density of the medium. This therefore relieves us of the necessity of assuming a single vortex filament.

There are some points still to be cleared up. For example, one might anticipate that the rotational velocity of the vortices would be the same as the translational velocity of the wave, but there appear to be at least one, and possibly two, other types, with rotational velocities of the square and cube root of the wave velocity; also in some respects the motion of what I have called the oscilla appear to differ from that of our standard vortex filament. All this is at present rather hard to interpret, but doubtless, as the difficulties of the analysis are gradually overcome, we shall be able to visualize the system more clearly.

As the work is still under way, the above results would not have been published but for the fact that it appears to have been generally assumed at the last British Association meeting that Planck's "quanta" theory and Maxwell's continuous medium theory are mutually exclusive and that one or the other must be given up. Now the results referred to above show that this is not so, but that every continuous medium theory *must* involve quanta, and we might almost say that a continuous medium begins to count as soon as it gets its legs. A unit quantity is, therefore, just as natural a thing as a flux; and in this connection it is interesting to note how, from Newton and Leibnitz down to Maxwell and Planck the English mind runs always to continuities and fluxes and the German to quanta and infinitesimals.

It may also be pointed out that quanta are a necessary consequence of motion due to central attraction. One visible example of this is the gaps in Saturn's rings. These are due to satellite resonance, but I have found that nucleus resonance gives quanta,[6] whether the resonant nucleus be the sun or the positive electron. The latter case is much the simpler, as all the corpuscles are the same size and so what we may call the "quanta orbits" are simpler.

From the above it will be seen that the problem of the transmission of plane waves in a frictionless fluid is not, as has been generally assumed, a matter of no practical importance and of interest to pure mathematicians only. But that it is a matter of very great practical importance, and that the complete solution of the problem is of capital importance in many fields, from the design of aeroplanes and the calculation of frictional resistance of ships to the theory of the constitution of the ether and the structure of the positive charge.

Reginald A. Fessenden

The Specific Gravity of Silt[1]

In a report recently published by the Department of State, entitled "Silt in the Rio Grande," certain fundamental ideas are promulgated, concerning the specific gravity of silt which seemed to the writer incorrect, and of sufficient importance to be worthy of a brief note in Science.

The author, W. W. Follett, consulting engineer of the International Boundary Commis-

[6] See also some of Darwin's papers.

[1] Published by permission of Director of the United States Geological Survey, Washington, D. C.

sion, and advisory engineer, commission for the equitable distribution of the waters of the Rio Grande, takes up the problem of how much space a given weight of river-borne silt will occupy when deposited in a reservoir, saying, on pages 11 and 12:

It was evident that the per cent. of bulk, obtained from test tubes, would be too large for the desired unit because there was no weight on the silt in the tube to compact it, as there would be in a reservoir. . . .

Something more than guesswork was wanted. It did not seem proper to us to found all our silt calculations on an assumed bulk for it which was, as it were, simply pulled out of the air. The desire was to approximate as closely as possible to the conditions which would be found in the bottom of a reservoir. After considering various schemes, to all of which there seemed to be valid objections, it was finally decided to seek a mud bar in the river where the water had been comparatively still and which had shrunk enough to show material cracks, and to cut from this bar a three-inch cube, have it dried out and weighed and to abide by the result, whatever it was. The idea was that a bar should be chosen which had shrunken enough to make up for the compression which the silt in the bottom of a reservoir would undergo from the weight of the water over it. Of course, the necessary amount of shrinkage could not be told exactly, but it was thought that a fairly good guess could be made.

The three-inch cube was collected, dried and found to weigh 85 per cent. as much as a three-inch cube of water. It was, therefore, assumed that "the above experiment fairly determined the weight of reservoir silt and that all silt determinations should be divided by 0.85 in order to obtain the actual final volume of the silt." The collection of the three-inch cube of silt is further described on page 75 of the report.

The first idea, which seems incorrect, is that deep water through its greater weight makes deposited silt more compact than shallow water. If the pores are filled with water, the pressure must be equal in all directions and the individual particles of silt being practically incompressible, the weight of the water must have negligible effect on the compactness of the silt. If the pores are not filled with water, but contain some air or other gas, the material would be compressed in proportion to the quantity of gas and the amount of pressure, but it does not seem probable that the compactness of silt is, in general, greatly affected by compression of included gases. It seems more reasonable to suppose that any greater compactness displayed by silt deposited in deep water is due to the arrangement of the particles or a modification of their form, brought about by the great distance traversed in settling, and especially is this true unless it can be shown that such silt expands when taken out of the water.

The second somewhat surprising idea is that one three-inch cube furnishes a better basis for determining the specific gravity of Rio Grande silt than all other available data, both inferential and experimental. If this be correct, there is certainly great need of adding to the available data, for the determination concerning the three-inch cube seems to be a small foundation for the argument and hundreds of computations which are based upon them. The result obtained, namely, that silt free from water weighs only 53 pounds per cubic foot, is considerably below most estimates and means that the material has a pore space of nearly 68 per cent. E. W. SHAW

ON PSYCHOLOGY AND MEDICAL EDUCATION[1]

FOLLOWING the symposium on psychology

[1] Report of the Committee of the American Psychological Association. The committee was constituted as follows: Shepherd Ivory Franz, scientific director and psychologist, Government Hospital for the Insane, and professor of physiology, George Washington Medical School, chairman; E. E. Southard, professor of neuropathology, Harvard Medical School, and director of the psychopathic department of the Boston State Hospital, and J. B. Watson, professor of psychology and director of the psychological laboratory, Johns Hopkins University. The scope of the inquiries of the committee was determined by the committee; the present report was written by the chairman, who is responsible for its form and the accuracy of its parts, but all the members of the committee are in accord with the conclusions.